a great safeguard to struggling medicos, the nurses could be kept in their proper place and honourable status, while the public would not be deceived into believing that the nurses are "registered" in a like manner as medical practitioners.

Those at the top or in the upper branches of the medical tree may think this idea is a distinction without a difference, but the general practitioner knows where the shoe pinches, and dreads the advent of yet another set of unqualified practitioners being supinely permitted by our lax legislation.—I am, etc.,

Malvern, Sept. 12th. STANLEY HAYNES, M.D.

KISSING THE BOOK.

SIR,—I was surprised to read Dr. Horace Dobell's letter in your issue of last week. Most of your readers will be more disposed to agree with the writer of a leading article in the *Times* of the 23rd ult., that our oath-taking in England is a nasty mode which is wholly without defence. That mediate contagion by such articles as blowpipes, tobacco pipes, glasses, and cups has occurred is beyond doubt; that a book may convey infection is manifest, though it would be very difficult to prove. More than forty years have passed since I first gave evidence; the handling and kissing of a book in indiscriminate use always appeared to me most objectionable. In 1884 I gave evidence in a case of rape; the first witness was severely affected with constitutional syphilis. I was sworn on the same book that she and all the other witnesses had kissed. I selected a clean page of the opened book and kissed that, but the book ought to have been destroyed and a new one used.

The Oaths Act of 1888 permits every one to take the Scottish oath, which has everything to recommend its adoption in England. Nothing could be more distasteful to me than to appear singular and to be for years the only witness swearing with uplifted hand in our local courts. It is some consolation for me to feel assured that the Scottish form of oath has come to England to stay there, and I trust that all associates and other readers of the BRITISH MEDICAL JOURNAL who agree with me will show that they have the courage of their opinion by swearing in future with uplifted hand.—I am, etc.,

Liverpool, Sept. 15th. FRED. W. LOWNDES.

RELATIVITY IN INFECTIOUS DISEASES.

SIR,—In your leading article this week commenting on Dr. F. M. Turner's report on return cases in London during the years 1902, 1903, and 1904, you were able to devote but one sentence to the difficulties encountered through cross-infection. Now, as this report of Dr. Turner deals with a larger number of cases than any other investigator has yet reviewed, and as Dr. Turner has handled his figures in a particularly able and satisfactory fashion, the whole study stands out as by far the most considerable contribution yet made to the elucidation of the tiresome problem of post-hospital infection. I therefore trust that the evidence with regard to cross-infection, and the significance it gains from its setting, will not lightly be overlooked. In a paper published some three years ago on the "Borderlands of Scarlet Fever and Diphtheria" I tried to insist on the relativity of the terms scarlet fever and diphtheria as disease-names, and to argue that while the entities they denoted were sufficiently definite for rough practical purposes, their full connotation was really very wide. I went so far as to say that, far from scarlet fever being scarlet fever *et praeterea nihil*, there was no reason why a scarlet fever patient should not, under certain circumstances, transmit diphtheria and other throat-infections to his neighbours, and that, *mutatis mutandis*, the same was true of diphtheria. I remember that this contention was rather sharply criticized at the time. It was asserted against me that these diseases were specific, that they "bred true," and that to think otherwise was pathological heresy. My hypothesis that infectious diseases were complex conditions, syndromes rather than specific diseases, required as one of the conditions of its establishment that a person ill, say, with scarlet fever should be able to infect others either with the principal element in his malady, namely, scarlet fever, or with one of the lesser factors contributing to that malady, say, diphtheria, pyogenous tonsillitis, or what not, as the case might be. It equally required, on the theory of probabilities, that he should infect more people with his principal infection than with his subordinate one or ones, and, moreover, that he should even occasionally distribute both infections simultaneously either to the same or different people. In Dr. Turner's report these three points are definitely established. Scarlet fever patients, of course, most frequently infected others with scarlet fever, and diphtheria patients most frequently infected others with diphtheria, but after carefully discounting all extraneous factors, Dr. Turner found that 0.28 per cent. of scarlet-fever patients on their discharge from hospital initiated attacks, not of scarlet fever but of diphtheria, and that 0.29 per cent. of diphtheria patients under similar circumstances set up outbreaks, not of diphtheria, but of scarlet fever; and, more interesting still, in twelve other outbreaks following the discharge of scarlet fever patients, one or more of the infected ones suffered from diphtheria and and one or more from scarlet fever, whilst in five outbreaks following the discharge of diphtheria patients some of the sufferers had one disease and some the other. Again, in the section of the report entitled "Recurrent Outbreaks in the Same House," may be read accounts of six outbreaks, in which the returned sufferers signalized their return to the domestic fold by distributing scarlet fever and diphtheria to their friends and relatives with a want of discrimination almost bloodthirsty in its impartiality. Indeed, the close investigation of these diseases showed them to behave with a complete and callous disregard of all the recognized canons of infectious morality.

Surely this evidence is sufficient to shatter the old idol of "breeding true." The term is an unfortunate one at the best, and it always invited the pickaxe of the iconoclast. If it indicates a doctrine at all, that doctrine is so much discounted by exceptions that the sooner it is scrapped the better. What has been shown to occur in the case of scarlet fever and diphtheria is certainly true also of typhoid fever, and Dr. Hamer's admirable Milroy Lectures persuasively suggested similar inferences with regard to other diseases.

Now, if scarlet fever can "breed" diphtheria, what conclusion are we to draw as to the character of this scarlet fever? That it is an unnatural father? Or that a spurious paternity has been saddled on its innocent back? I think neither; I should prefer merely to consider that it leads a double life. Sometimes it comes in the garment of respectability, and sometimes, like the ghost of Hamlet's father, in questionable shape. When the main elements of the disease predominate, we all recognize it; when these elements are closely associated with others its real nature may be obscured, difficult or even impossible to recognize. In fact, in these infectious diseases, certainly also in typhoid fever, and probably in all, a complicated process of symbiosis is present, the various elements concerned reacting, these with those, and all with the tissues of the host. The summation, the resultant, the lowest common multiple, or whatever it is best called, produces the "clinical picture." Considering the numbers of artists at work, it is not surprising that some Whistler at times presents us with a clinical "nocturne" which we find it difficult to hang in our conventional academy and which brings despair into the heart of the catalogue maker. But it is just these eccentric productions that often supply the clue that leads us to understand the true inwardness of the conventions.—I am, etc.,

Outer Temple, W.C., Sept. 14th. HUBERT E. J. BISS.

RETURN CASES OF SCARLET FEVER.

SIR,—In your issue of September 15th, referring to my report on return cases, you state that I believe "that there is good reason to hope that by sending all cases to convalescent hospitals before discharge the total infectivity rate might be reduced by two-thirds."

In my report, however, I state "past experience leads to the hope that return cases might be reduced to about two-thirds or less of the present number by discharging all cases from the convalescent hospitals."

May I ask you to publish this correction?—I am, etc.,

South-Eastern Hospital, S.E., Sept. 17th. F. M. TURNER.